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AN ASPECT OF JUDGMENT IN TAXONOMY

by

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This is a statement of a fundamental assumption of the author concerning the method of taxonomy. It is offered, as a contribution to the discussion of such subjects which he believes should be undertaken to clarify this and similar concepts in taxonomy. The assumption concerns the use a taxonomist should make of the facts he derives from herbarium specimens to decide how much variation he should include in the taxon exemplified beyond that shown by those specimens. It might be thought this aspect of judgment should come under the general survey of the application of statistics to taxonomy but the paucity of herbarium material usually available presents a special problem. Such material is most often too sparse and wrongly selected to make up a sample suitable for orthodox statistical treatment.

The opinion of a taxonomist on the matter discussed below affects any study of a plant group using preserved specimens; it was of particular importance in the making of one decision in the author's study of *Cassipourea* in Africa [1]). This decision concerned the possible specific distinctness of *C. nodosa*; it is included below as an example of the way in which the adoption of this fundamental assumption affects judgment in taxonomy. Although a comparatively inexperienced taxonomist, I may add that this article has been discussed with several of my colleagues at Kew and I am indebted to them for many helpful comments from which the presentation of the following argument has gained in precision.

Introduction: The very restricted view.

Herbarium specimens of a postulated taxon are a sample, often inadequate, of a population in the field, the members of which may or may not exceed in the variation of their characters (i.e. of their morphological and other features) the range shown by the collected specimens. The taxonomist is forced to consider the degree to which the variation of this population may exceed that of the sample that he has examined. This problem is especially acute when only one specimen is available which differs in a number of characters from all other known examples of the taxon to which it may belong. The taxon wil usually be an even larger population than the field population from which the specimen comes, including usually a number of such populations. Two assumptions may be made. First that within the major population (which is the taxon), there exists a subpopulation (in the field) of which the specimen is a sample; secondly, that the members of this sub-population resemble the specimen exactly in those characters which distinguish

1) See Kew Bulletin 1955 — four East African species and their synonyms throughout Africa.

the specimen within the major population from other subpopulations. If both these assumptions are made the sub-population will be immediately acceptable as a separate nameable taxon. The level within the taxonomic hierarchy at which its name is to be used will depend only upon an author's view of the number and value of character-differences needed in the group being studied to distinguish taxonomic groups at the different levels. It is in the adoption of such assumptions that a taxonomist develops that attitude to his work which is referred to as his taxonomic judgment. It is upon the second of the above assumptions that I wish to focus attention. The judgment of a taxonomist who adopts it will have a very restricted basis; nothing will be inferred to be true of the sub-population(s) beyond that which has been observed in the specimen(s) examined. It is to the use of this method in taxonomy that the present writer is opposed.

Thesis: The inductive view.

The present writer believes that a taxonomist should attempt to provide a classification that will apply to plant life as a whole, not merely to those specimens which have been collected. Hence, only at the lowest level (or very rarely, levels) of taxonomic significance [2]) should a taxon either comprise or take its circumscription from a single individual, and even at the lowest level(s) it should do so from more than one specimen if the individual is clonal. That the reservation in this last sentence applies (very usually) only to a single level is due to my preferring to consider forms, i.e., the smallest groups in the taxonomic hierarchy, as purely taxonomic groups, ecads (ecological forms) being below this level. Therefore, for me, varieties (very usually) as well as species, as dealt with in taxonomy, are abstractions from a number of individuals which exist [3]) in nature, while forms may or may not be.

All the individuals from which the abstraction is made that circumscribes the taxon may or may not have been examined. This means that the recognition and description of taxa, at all but the lowest level(s), is less valid, but not impossible, when derived from single or few specimens. Taxonomists should recognise that, when this is contemplated, allowance should be made for the variation possible in the whole subpopulation. It cannot be denied that all taxonomists do make this allowance to some extent; for example, no-one believes that, in general, the exact height of a single example of a taxon will necessarily be equalled by that of any subsequent gathering. The distinctiveness of the present author's view is in his belief that every character must be consciously considered under this aspect of its variability. This is the basic assumption in support of which this article is written. This assumption involves the taxonomist in supplying from his imagination that part of the variation which may be expected to occur in the unseen part of the subpopulation, i.e., that part of the variation beyond the sample. In order to do this validly he makes use of his knowledge of the variability found throughout the larger group of which the subpopulation envisaged is a part, and that of taxonomically closely related groups. His conception of the limits in the variation of characters in the hypothetical population is thus the result of an act of induction and, as such, is no less scientifically respectable than the very restricted conception dealt

2) Characters of taxonomic significance are other than those distinguishing phenotypes which have a common genetical constitution; "level" refers to a grade in the nomenclatural hierarchy used in taxonomy.

3) or have existed in the case of a solitary relict, or will exist in the case of the solitary initiator of a future population. If an individual is the sole possessor of its characters that have existed or will ever exist, it should not be taken to represent a taxon above the level of a form, or very exceptionally of a variety.

with in the previous section. It must be mentioned that this act of induction is *also* based upon induced generalisations about the pattern of organic evolution, but these are more generally agreed and to discuss them in detail would unjustifiably enlarge the scope of this article. In such generalisations would be included the factors due to the geographical area concerned. Variability may not be uniform throughout the world; whether the group concerned is temperate or tropical in its distribution, for example, may affect the degree of variation postulated in the hypothetical subpopulation. Again any knowledge of the presence of barriers to interbreeding or to the spread of disseminules will be taken account of in this way.

A practical illustration.

An example of the application in practice of the above theoretical consideration will now be given. It affected a decision which had to be made concerning the affinities of some of the African members of the tropical and subtropical genus *Cassipourea* for the family *Rhizophoraceae* in the Flora of Tropical East Africa. In *Cassipourea* subgenus *Dactylopetalum* a species has been described by mr. A. H. G. Alston under the name *C. nodosa* [4]). The type specimen is from near a river estuary on the Atlantic coast of Africa, six degrees south of the Equator, on the border between Belgian Congo and Angola, at Santo Antonio do Zaire, *Gossweiler* 8759 (K, holotype !; BM, isotype!); it is the only known gathering. Within the subgenus concerned there is also a large and very variable group of plants distributed widely in tropical Africa to which *C. nodosa* is undoubtedly allied by virtue of the majority of its characters. The present writer considers this group to be referable as a whole to the species *C. gummiflua* Tul. [5]) of which the type specimen is from Madagascar, Nossibé island, *Boivin* (P, holotype!; K, ? isotype!). Whether this group is properly to be referred to one species need not concern us here; the name "*C. gummiflua*" will be used as a name for the group in discussing whether *C. nodosa* is to be considered taxonomically as within it or separable from it.

Analysis of the characters of *C. nodosa* and "*C. gummiflua*" shows that those of the former fall within the range of variation of those of the latter in all but three characters. It must be emphasized that since 1925, when Alston described *C. nodosa*, although no more material of his species has been collected, the known range of variation of "*C. gummiflua*" has been increased by recent gatherings. The three characters in which *C. nodosa* exceeds the range of variation of "*C. gummiflua*" are best shown in tabular form.

	C. nodosa	"*C. gummiflua*"
Number of calyx-lobes	(?5 or 6-)7(-8)	4-6(?-7)
Length of pedicel in mm.	0.5	1-4
Number of flowers per leaf-axil	10-40	3-20

The apparent overlap in the number of calyx-lobes is questionable, as indicated. *C. nodosa*, in eleven flowers dissected, showed nine with seven-lobed (regular) calyces and two with eight-lobed (irregular) ones. In "*C. gummiflua*" I have found only four to six lobes on the calyces of the very many flowers examined (nine previously recognised "species" are included in my conception of *C. gummiflua*). A seven-lobed calyx is reported in one of the members of "*C. gummiflua*" but like the original description of *C. nodosa* as having five or six calyx-lobes I consider this report suspect.

4) Kew Bull. 1925 : 273 (1925) — a monograph of the genus.

5) Ann. Sci. Nat. sér. IV, 6 : 123 (1856); Alston in Kew Bull. 1925 : 272 (1925); Lewis in Kew Bull. 1955.

Given the facts in the above table the taxonomist who adopts the very restricted view mentioned first above would undoubtedly recognise *C. nodosa* as a species distinct from "*C. gummiflua*".

The adoption of the less restrictive assumption allows us to reason as follows. If *Gossweiler* 8759 is a specimen from the only tree with its characters which has existed, exists or will exist, the taxon it alone composes cannot be ranked as a species, see footnote 3 above. We must consider, therefore, the variation of the subpopulation (within the population that comprises both *C. nodosa* and "*C. gummiflua*") of which *Gossweiler* 8759 is a sample.

General knowledge of the subgenus allows us to induce that the other members of the subpopulation would not necessarily combine the extremes in the three characters in the way that this specimen does. Each character is likely to vary independently. To consider first the number of flowers. In the subgenus the nodes of the stem bear flowers perennially in fascicles and from all the material the generalisation may be formed that the number of flowers depends, at least in part, on the age of the stem. *Gossweiler* 8759 is the only specimen with a stem greater in diameter than 4 mm.; just below the node bearing forty flowers (or pedicels) per axil it is 6 mm. in diameter. Therefore we may conclude that a young piece of stem from a tree of the same genetic constitution as *C. nodosa* might well have fewer than ten flowers per axil, and that an old stem from a tree otherwise referable to "*C. gummiflua*" might well have more than twenty per axil. Concerning the length of the pedicels, it is noticeable that the flowers of *Gossweiler* 8759 are all in very young bud. Since in "*C. gummiflua*" some specimens show an increase in pedicel-length between the young and the mature flowers, we can induce that in the subpopulation including *C. nodosa* mature flowers might well occur having pedicels 1.0 mm. long. Throughout the whole population of "*C. gummiflua*" and *C. nodosa* together the length of the pedicel is not, perhaps, wholly determined by the age of the flower but it is sufficiently so to reduce markedly the taxonomic significance of the short pedicels on *Gossweiler* 8759. Similar considerations apply to the difference in the number of calyx-lobes; here again the distinctness of *C. nodosa* is but slightly significant, especially if the overlap in the numbers, which I cannot confirm, was evident in the collected samples in the past. Enough has been said to show how the conclusion has been reached that the occurrence of the three extremes of character-variation on the one specimen *Gossweiler* 8759, is judged to be merely coincidental. Readers who wish to know the affinity of *C. nodosa* within "*C. gummiflua*" may refer to the author's article in the Kew Bulletin.

I hope I have shown by this comparatively simple example the sort of reasoning process that should be undergone in making a taxonomic judgment, and the way in which this process arises from a fundamental assumption in taxonomic method. It may be objected that this example could equally well be regarded as an example of an improvement in taxonomic treatment due to recognising that two herbarium samples represent phases of growth of the same species. Attention is often drawn to this source of error by saying that examples of two taxa can only be validly contrasted if they are in the same physiological state, or that the comparison of the organs of two specimens can only lead to valid conclusions if they are at the same stage of development. That the example given above is partly of this kind is true; it is admittedly a somewhat special case. The variation in appearance of specimens due to degree of development is, however, only one cause of the differences between specimens of phenotypes with the same genome. Failure to recognise developmental stages is one of the errors inherent in the restricted method. The adoption of the fully inductive view means that the discontinuities in the range of variation of specimens which are used to define taxa, at all but the lowest level(s),

must be such that they could not be caused by environmental factors, disease, preparation of the specimens and stages of growth.

A further consideration.

There is a subsidiary conclusion which follows from the adoption of the inductive view analysed above and which may be suitably included here. The amount of experience a taxonomist has of a major population will affect the validity of his recognition of a sub-population thereof, as represented by a sample, as a new taxon. For example, in order to recognise a new species validly by certain characters he must know the range of variation of these characters in all the species composing the genus, at least in sufficient detail to be sure that this range is exceeded by that of the new species and he must be familiar with the "variation pattern" of the genus, i.e., know the relative value of different character-differences in the genus. The use of combinations of characters will not affect this conclusion materially; the same considerations apply to such combinations.

This conclusion that experience of plants other than those of the group immediately being studied is vital to the validity of taxonomic treatment would be agreed to by many taxonomists. My purpose in including it briefly here is to emphasize that it depends upon the taking of an inductive or imaginative view of character-variation. On the restricted view it would be sufficient merely to find differences of any degree between specimens in the herbarium in order to justify the description of new taxa; in order to show this limited approach to be false, it is necessary to admit the acceptance of some way in which experience beyond the sample can affect taxonomic decisions. The way in which it does so is the inductive method referred to.

Conclusion.

It has been suggested to the author that no modern taxonomist adopts the restricted view given first above. This may well be; the author is, however, concerned to advocate the deliberate adoption of the fully inductive view which is a different matter. It is not enough occasionally to avoid designating a new species because intermediate forms are known to exist, it may be necessary to postulate that unknown intermediate forms occur. Evolution is a process of differentiation, hence of any two specimens the initial assumption to be made is that they were collected from plants which were not genetically distinct, the onus is on the taxonomist to prove that evolutionary differentiation has taken place. The taxonomist should always ask himself if there be not some way in which his botanical knowledge as a whole can causally account for and so reduce in taxonomic significance the differences that he observes in specimens. If there be not, then those differences are probably of taxonomic importance. The argument is closely bound up with the subject of botanical terminology since a "character" is in essence, merely a descriptive phrase; the fact is that "differences" can be found between *any* two specimens or groups of specimens and it is necessary to adopt some criterion of judgment regarding the taxonomic value of the different degrees of difference. This article attempts to give a formal analysis of the best way to do this.

Note: As was said at the beginning the author believes that discussion of the basic assumptions of taxonomy is overdue. This article introduces one topic of this kind. If it be true that his "inductive view" is the common method of present day taxonomy, as has been suggested to him, then at least this common method is analysed here for the use of teachers of taxonomy. On the other hand, some of the author's colleagues have not agreed that his analysis represents a worthwhile method in taxonomy, which indicates that some discussion may well ensue. Criticisms of all kinds will be welcomed by the author and may be addressed to him care of The Director, Royal Botanic Gardens, Kew, England.

THE AMERICAN COLLECTIONS OF HUMBOLDT AND BONPLAND, AS DESCRIBED IN THE SYSTEMA VEGETABILIUM OF ROEMER AND SCHULTES

by

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The most important botanical collections ever made in tropical America, from the standpoint of taxonomy, were probably those made by Alexander von Humboldt and Aimé Bonpland, from 1799 to 1804. The importance of the collection stems, of course, not from the large number of specimens which was obtained, nor from the regions in which the specimens were collected, but particularly from the fact that, through the genius of Humboldt and the diligence of Bonpland and Kunth, the results of the expedition were made known through adequate — indeed sumptuous — publication, within a comparatively short time.

Much has been written about the collections and publications of Humboldt and his collaborators, both by contemporary botanists and by more recent workers, but today it is not generally realized that during the process of publication of the hundreds of new plants, an unfortunate chain of circumstances resulted in a series of competitive publications that tried the tempers of the botanists who were concerned, and introduced a series of arguments — still unsettled — over priority in nomenclature.

To most botanists today, the names Humboldt and Bonpland suggest the initials *H.B.K.*, which stand for the authors of the monumental seven-volume *Nova Genera et Species Plantarum* (1816—1825) in which most of the new American species were published. The names of the two collectors also suggest the volumes prepared by Bonpland and published under what amounted to joint authorship, namely the two-volume *Monographia Melastomacearum* (1806—1823) and the two-volume *Plantae aequinoctiales* (1805—1818). Many of the new species however, were published by other authors. Karl Sigismund Kunth, the principal author of the *Nova Genera et Species*, published also a large folio work on the Leguminosae collected by Humboldt and Bonpland. The celebrated J. G. C. Lehmann, of Hamburg, described some of the Boraginaceae and Solanaceae. Willdenow himself, Humboldt's friend and informal mentor in botany, published certain species as early as 1805, in the fourth volume of the *Species Plantarum*. Finally may be mentioned the large group of species (more than 250) published by Roemer and Schultes. I shall have more to say of these, but first a word about the fate of the actual botanical specimens collected by Humboldt and Bonpland.

The botanical collection of more than 6.000 specimens was offered to the Muséum National d'Histoire Naturelle, of Paris, in December 1804, and was at once accepted, after consideration by a committee consisting of Jussieu, Lamarck and Desfontaines. Apparently because of Humboldt's wish in the matter, however, the obligation to study and report on the collection was undertaken by Karl Ludwig Willdenow, of Berlin. In addition to being a personal friend of Humboldt, Willdenow was in a way the logical person to undertake such a task. He was not an old man (about 40 years of age), and he wore nominally the mantle of Linnaeus, having completed

1) In the preparation of this paper I have had much assistance from my colleague, Eileen M. Squires, who has painstakingly prepared translations of certain Latin passages quoted below, in which Kunth, Humboldt and Schultes expressed themselves freely and positively. Latin as a medium of expression differs so markedly from the jargon used by botanists that I feel myself completely incompetent to interpret shades of meaning and emotion in ordinary prose, and I am grateful to Mrs. Squires for her cooperation.

the major part of the publication of a revised (4th) edition of the *Species Plantarum*. Humboldt had sent to him from Cuba, in 1801, in the care of John Fraser, herbarium specimens representing some 1.600 species [2]). Then about the end of the year 1805, a little over a year after his return to Europe, Humboldt went to Berlin and spent some time with Willdenow, sorting and arranging the American plants.[3]) Perhaps it was at this time that final arrangements were made for Willdenow to continue work with all collections that Humboldt and Bonpland did not wish to handle themselves. Before this, of course they had begun to work on the *Plantae aequinoctiales* and the *Monographia Melastomacearum*, the first parts of which were published in 1805 and 1806, respectively.

It is clear, in any event, that Humboldt gave a very large set of the American plants to Willdenow with the understanding that the latter be responsible for the elaboration of the botanical results of the American expedition. According to Urban, Willdenow planned a new handbook with improved descriptions, to include all known plants. He spent a winter in study among the rich collections at Paris, then as now the botanical center of the world, and at least a part of his work was financed through Humboldt's efforts [4]). He died in 1812, however, not long after his return from Paris, leaving mere fragments of his projected work, but many brief descriptions and new names attached to the specimens in his herbarium.

Humboldt, although grieved at Willdenow's death, and fearful lest this postpone indefinitely the publication of the work, enlisted the services of Kunth, who at this time was a young man of 24, already known for his studies on the flora of the vicinity of Berlin. At the time of the publication of the first fascicles of the *Nova Genera et Species*, probably early in 1816, Kunth had spent two years in Paris working with the collections. Unfortunately, as it seemed to Kunth and as it seems to us, the complete collections of Humboldt and Bonpland were never fully available to him. He had access to the original set in Paris, but for some reason, as yet unexplained, he did not have full access to the Willdenow herbarium. In Humboldt's general statement of the matter [5]) he says merely: "[Kunth] igitur schedarum et observationum Willdenowicarum *partem* in suos usus convertit" [italics mine].

I have not been able to learn why the collections in the Willdenow herbarium were not seen by Kunth during his many years of work on the American plants. It may have been because of some personal reason, perhaps because of some nationalistic feeling on the part of those who inherited the herbarium, or perhaps because Kunth at first supposed the Paris collection to contain examples of all the species found by Humboldt and Bonpland. As soon transpired, however, Willdenow's set contained not only duplicates of most of the collections in Paris, but also some unique specimens not represented in the principal set. Kunth certainly knew of the existence of these unique specimens as early as 1816 [6]). In a note a few years after this [7]) he implied that he had wilfully been denied the materials in the Willdenovian herbarium; in referring to the elder Schlechtendal and his work with the herbarium,

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2) Humboldt, Alexander. Lettres américaines d'Alexandre de Humboldt (1798—1807). (E. T. Hamy, ed.). pp. I—[XL], 1—309. Paris, [?1905]. cf. pp. 108, 155.

3) Humb. l.c. 210.

4) Urban. I. Gesch. K. Bot. Mus. Berl. (1815—1913). pp. 13—14. Dresden, 1916; Humb. l.c. 258.

5) Humb. in H.B.K. 1: V (proleg.) 1816.

6) cf. Flora 3 : 257—271. 7 May 1820. This is an anonymous article entitled: *Einige Bemerkungen zu Römer und Schultes Systema vegetabilium (Ein Wort zu seiner Zeit)*. It contains a rather lengthy account of the situation discussed in the present paper, and its propriety, as well as its effects upon botany.

7) Kunth, Syn. Pl. Aeq. 1 : ii. 1822.

Kunth wrote: "Plantas Humboldtianas ibi repertas, nobis non consentientibus, licet semper ad consentiendum paratissimis, cum aliis botanicis communicavit". Kunth made another, similar implication in another place [8]), after deploring the indiscriminate and unscholarly way in which Willdenow's notes had been published: "Judicent nunc botanici, quo auxilio schedae Willdenowianae mihi fuerint, quatenus iis usus sim et potuerim uti".

On this matter of the right of access to the collections of Humboldt and Bonpland, Kunth felt strongly that his report was to be, in a sense, the official one, and that a breach of ethics was committed by any other botanists who published new species based on the same collections, before the completion of the *Nova Genera*. This may seem a reasonable point of view, but it must be remembered that the collections had been in Europe more than a decade before the *Nova Genera* began to appear in print, and it was equally reasonable for monographers, and compilers of general floristic works like the *Systema Vegetabilium*, to become impatient. In any event there were "leaks", apparently all from the Willdenow collection, which was not under Kunth's control.

Willdenow's herbarium was purchased by the Berlin Botanical Garden, and came into their possession in 1818, but before this it passed after his death for six years into the custody of his friend D. F. K. von Schlechtendal (1767—1842), the father of the botanically better-known D. F. L. von Schlechtendal (1794—1866). The elder Schlechtendal, according to Urban, devoted all his spare time to the improvement of the herbarium but, because of the very large amounts of un-named and unclassified material which it contained he was unable to complete the task of bringing it to order. (As an example of this, when Humboldt initially went over the American collections with Willdenow, they estimated that three-fourths of the species were new to science [9])). Schlechtendal therefore enlisted the cooperation of some specialists (e.g. Lehmann), and about the same time he opened the collections for use by Roemer and Schultes and allowed them to appropriate and publish, in their *Systema Vegetabilium*, the names and often imperfect descriptions left unpublished by Willdenow.

Johann Jakob Roemer and Joseph August Schultes began publication of their *Systema* (a "new edition" of Murray's 1774 work of the same name, and supposedly a lineal descendant of Linnaeus's *Systema Naturae*) in 1817. In this year they issued the first two volumes, comprising treatments of vascular plants according to the Linnaean system, through *Triandria trigynia*. Volume 1 was apparently issued early in the year; the preface was dated December 31, 1816. In this volume, as far as I am aware, the only references to the plants of Humboldt and Bonpland are in a few citations of pages in Volume 1 of the *Nova Genera*, up as far as page 260 (this fascicle published in August, 1816). The second volume of the *Systema* seems to have been published about seven months after the first; its preface is dated July 31, 1817. Its pages are devoted almost entirely to the grasses and sedges, and there are numerous references to the species published in Volume 1 of the *Nova Genera*, but no mention, a far as I can tell, of any unpublished species from the Willdenow Herbarium.

In the preface to volume 3 of the *Systema*, however, under date of March 20, 1818, the authors acknowledge their debt to "L. B. de Schlechtendal, qui Willdenowiani Herbarii thesaurum humanissime nobis aperuit". Schlechtendal seems to have opened the Willdenovian treasures to Roemer and Schultes rather shortly before this note was written, because no mention of Humboldt's specimens is found in the body of the volume. In the list of *Addenda et Emendanda* at the end of the book, however,

8) H.B.K. Nov. Gen. & Sp. 3 : 352 (folio). 1820.

9) Humb. Lettres Amér. 210.

there are described about 27 species from Mexico and South America, based on Humboldt specimens from the Willdenow Herbarium. These species include 6 in the genus *Hedyotis* (pp. 526—527), 3 in *Houstonia* (p. 527), 12 in *Spermacoce* (pp. 530—532), 2 in *Knoxia* (p. 532), 1 in *Diodia* (p. 532) and 4 in *Embothrium* (p. 533). In the *Index kewensis* these species were given rather summary treatment, and many of them seem to have been suppressed in favor of the corresponding names published more than a year later, in Fascicle 12 of the *Nova Genera et Species*. Some of the R. & S. names, however, have been taken up and used by later botanists, and most of the South American Rubiaceae have been satisfactorily accounted for by Standley [10]). The Proteaceae (*Embothrium*) were apparently published a few weeks later than the corresponding species in the *Nova Genera*.

The fourth volume of the *Systema* was issued early in the year 1819, and with it came a flood of new species from the Willdenow herbarium. In this volume, and in Volume 5 which followed it less than a year later, Roemer and Schultes treated the large Linnaean group *Pentandria monogynia*. Unfortunately for posterity, and botanists who are concerned with priority in nomenclature, Volume 3 of the *Nova Genera* was issued in 5 fascicles, the earliest of which (S 1818) appeared before Volume 4 of the *Systema*, and the latest of which (Mr 1820) was published after Volume 5 of the *Systema*. By a coincidence the order of publication followed in the *Nova Genera* [11]) had brought it to the group of dicot families with regular gamopetalous flowers, most of which have some or all of their members in *Pentandria monogynia*. As a result of this, many species in several large American groups, e.g. Solanaceae, Boraginaceae, Convolvulaceae, Campanulaceae (mostly Lobelioideae) and Rubiaceae, were described independently by two different authors, nearly at the same time, but often under different names.

Confusion with respect to priority is, of course, a comparatively recent development. At the time the person most deeply involved was Kunth, who cared little for formal priority but felt strongly about the "misuse" of the Willdenow herbarium. In Kunth's view the release of the Humboldt and Bonpland specimens to Roemer and Schultes, and to others, compounded a situation which was highly irregular. In the first place, as he said, the hastily drawn diagnoses which were published under Willdenow's name were often sadly lacking in essential features; at best they were too brief, and at worst they were misleading or actually erroneous. Some species were assigned to the wrong genus or even the wrong family. Species published in this way reflected credit neither upon their authors, nor upon the memory of Willdenow. In the second place, Kunth felt that publication of the new species in other places, by unauthorized persons, in competition as it were with the *Nova Genera et Species Plantarum*, was likely to multiply greatly the number of synonyms in the literature and create confusion in science, and so was not only unethical but unwise.

After the publication of Fascicle 9 of the *Nova Genera*, in September, 1818, Kunth received the *Generis Nicotianarum historia*, by Lehmann, and Part I of the *Plantae e Familia Asperifoliarum nuciferae*, by the same author. Both these works had appeared in 1818, prior to the publication of Fascicle 9. Kunth apparently learned for the first time that Lehmann had been working on revisions of *Nicotiana* and of the Boraginaceae, and realized, also for the first time, that Lehmann had described new species from the Humboldt collections. Some had been published as early as 1817 in periodical publications.

10) Field Mus. Publ. Bot. 7 : 1—504. 1930 - 31; l.c. 13(6) : 1—261, 1936.

11) Neither Humboldt nor Kunth liked the artificial system of Linnaeus: "In recensendis plantis systemati artificiali Linnei pulcherrimam Jussaei distributionem plantarum secundum tribus naturales praetulimus" (H.B.K. Nov. Gen. & Sp. 1 : V [proleg.]. 1816).

Apparently more in sorrow than in anger, Kunth wrote a mild protest to Lehmann [12]). The more pertinent parts of his letter are given in translation:

> Would that I had known your plan earlier, so that I could have sent you what I have of this family. Herr von Humboldt has always allowed me to do this in similar cases ... The least that Herr von Humboldt can ask is that in return for many sacrifices made, he be allowed to publish his plants himself. Understand that I do not mean to reproach you, least of all since you seem not once to know the discoverer of these plants. It is most unfortunate, however, that these valuable materials do not always find such a distinguished observer and skillful worker as yourself; I have seen to my great sorrow how they are permitting the publication of mere provisional names and hastily drawn diagnoses from the Willdenovian herbarium, without considering how much communications of this kind have damaged the botanical reputation of this otherwise meritorious man, and will damage it further.

The last few lines of Kunth's letter doubtless refer to the new species which had appeared in the third volume of the *Systema Vegetabilium*, published in the spring of 1818. As pointed out above, several Humboldtian species of Rubiaceae, first described in that volume, have priority over their Kunthian equivalents in Volume 3 of the *Nova Genera*. As far as I have learned, however, Kunth did not refer to these species specifically in later publications, nor did he prepare for Volume 3 of the *Systema* any list of synonymy as he did for the later works of Roemer and Schultes.

In 1820 Kunth wrote and edited a second, and stronger, protest, accompanied by a list of about 235 species "pirated" by various authors ("Index plantarum nostrarum, a Lehmanno, Römerio et Schultesio sub aliis nominibus vulgatarum"). This was published in the last fascicle of the third volume of the *Nova Genera* [13]). Humboldt himself was fully aware of the controversy, as may be judged from the following extract from a letter to his brother, written April 21, 1820 [14]):

> Nous avons ajouté — au troisième volume des *Nova Genera* — une plainte semblable contre Schlechtendal qui, sans ma permission, donna mes plantes avec de mauvaises descriptions à M. M. Römer et Schultes, qui publient mes Species. Il en résulte que les mêmes plantes sont publiées deux fois sous différens noms.

A reply to Kunth was included in the sixth volume of the *Systema* [15]). Here Schultes (Roemer had died in January 1819) justified the publication of the Humboldt and Bonpland species by stating that such publication was in the nature of a debt owed to Willdenow [16]).

In 1822 appeared the first volume of Kunth's four-volume *Synopsis plantarum ... collegerunt Alexander de Humboldt et Amatus Bonpland*, and in the preface of this

12) Flora 1 : 601—607. 20 D 1818. Pp. 601—604 comprise: *Schreiben des Hrn. Dr. Kunth in Paris an Hrn. Prof. Dr. Lehmann in Hamburg*; Kunth's letter was dated 11 Oct. 1818. Pp. 604—607 comprise: *Antwort-Schreiben des Herrn Prof. Dr. Lehmann in Hamburg, an Hrn. Dr. Kunth in Berlin*; Lehmann's reply is dated 16 Nov. 1818. The letters were also published in Isis [von Oken] 1818 (Heft 10) : 1790—1792. 1818.

13) H.B.K. Nov. Gen. & Sp. 3 : 352—356 (folio) Mr 1820.

14) Humb. Lettres amér. 279.

15) R. & S. Syst. Veg. 6 : VI [of tile page]. Dated Calendis Augusti, 1820.

16) Nos vero, quid in rebus botanicis moris sit, et scire et fecisse nos credimus; peccasse enim in nostram tam L. B. *de Humboldt* et *Bonpland*, quam divi *Willdenow*, meritorum immortalium reverentiam videremur, si conticuissemus quae ab illis detecta, ab hoc licet nimis breviter descripta erant, quamprimum haec ab amicissimo ipso divi Willdenow viro, illustri de Schlechtendal, eô cum J. J. Römer communicata fuissent fine, ut publici juris fiant.

work (pp. i—iii) he again took occasion to complain against Schlechtendal and Link for what he considered to have been the abuse of Willdenow's trust.

A final word on the confusion appeared in 1830 [17]), this time another index by Kunth to the species of Roemer & Schultes and their equivalents in the *Nova Genera.*

The significance of the entire situation, as it affects botanists of the present day, may be summed up with a table showing the dates of publication of the several works in question:

H.B.K. *Nova Genera et Species*	R. & S. *Systema Vegetabilium*
Volume I. Ja—Au 1816	
	I. 1817 (? early Spring)
II. Ap 1817—Je 1818	
	II. 1817 (?Autumn)
	III. 1818 (?Spring)
III. Fasc. 9 S 1818	
10 [early] F 1819	
	IV. 1819 (?Spring)
III. Fasc. 11 Jl 1819	
12 N 1819	
	V. 1819 (?December)
III. Fasc. 13 Mr 1820	
IV. Ap—D 1820	
	VI. 1820 (?Autumn)
V. 1821—1823	
VI. 1823—1824	
VII. 1824—1825	
	VII. 1829...

It is evident that the conflicts over priority involve chiefly the names published in Volume III of the *Nova Genera.* Kunth's list of synonyms, published at the end of the index to Volume III (see note above), cited about 230 species from volumes 4 and 5 of the *Systema.*

Volume 4 of the *Systema* was almost certainly published early in the Spring of 1819; it was nearly complete by the middle of February or before [18]), and to judge by the speed with which it was followed by volumes 5 and 6, there was little delay in its publication.

It may be assumed, therefore, that Volume 4 of the *Systema* was published after Fascicle 10 of the *Nova Genera* (F 1819) and before Fascicle 11 (Jl 1819). In terms of pages, this means that pages 1—192 (folio, 1—148) of the *Nova Genera,* Volume 3, were published prior to the issuance of the *Systema,* Volume 4. Actually it appears,

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17) Synonyma ad plantas Humboldtianas e Mantissa tertia Roemeri et Schultesii relata a Car. Sigism. Kunth. Linnaea 5 : 366—369.

18) Flora 2 : 126. 28 F 1819. When this note was written, 50 signatures [*Bogen*] had already been printed. The text of the entire volume includes 51 signatures, of which the first 50 comprise pages 1—800. The first 44 signatures and the first page of the 45th (pp. 1—707) are devoted to the treatment of the first half of the *Pentandria monogynia.* The rest of the volume comprises a list of additions and emendations, chiefly species from Lehmann's treatment of the *Asperifoliae* (which the authors state they received when the text of volume 4 was partly set in type), species from volume II of the *Nova Genera,* and new species from the Willdenovian herbarium. The additions are concluded in the 51st signature (pp. 801—814) which also includes the first pages of the index to the volume.

from Kunth's list, that in most cases the H.B.K. names have priority. The exceptions are:

Cantua fasciculata R. & S. 4 : 369; (*Fouquieria spinosa* H.B.K. 3 : 452).
Echites mollis R. & S. 4 : 795; (*E. macrophylla* H.B.K. 3 : 170).
Apocynum cumanense R. & S. 4 : 796; (*Cynanchum fimbriatum* H.B.K. 3 : 158).
Jacquinia mucronata R. & S. 4 : 802; (*J. pubescens* H.B.K. 3 : 197).
Bumelia micrantha R. & S. 4 : 802; (*Ardisia micrantha* H.B.K. 3 : 193).
Ardisia arguta R. & S. 4 : 804; (*A. arguta* H.B.K. 3 : 194).
Rauwolfia laevigata R. & S. 4 : 805; (*Tabernaemontana jasminoides* H.B.K. 3 : 176).
Rauwolfia ligustrina R. & S. l.c.; (*R. ternifolia* H.B.K. 3 : 181).
R. viridis R. & S. l.c.; (*R. psychotrioides* H.B.K. l.c.).
Cestrum spermacocifolium R. & S. 4 : 808; (*Bouvardia triflora* H.B.K. 3 : 301).

It should be noted also that there are about 46 species, for which Kunth was unable to give equivalents, published by Roemer and Schultes in this volume. Presumably some of these were represented by specimens in the Willdenow herbarium but not in Paris; doubtless some others remained unidentified, as Kunth said, because of the brief descriptions published with them, or even because they were assigned by Roemer and Schultes to the wrong families or genera.

Volume 5 of the *Systema* seems to have been published late in the year 1819. The title-page is dated 1819, the preface is dated "23 Sept. 1819". A published note [19]), based on a communication written in November, 1819, states that at the time of writing Volume 5 was complete except for a few signatures, so that presumably the actual date of issue was some weeks later, perhaps in December or at least late November.

As Barnhart has pointed out [20]), Fascicle 12 of the *Nova Genera et Species*, comprising pages 289—384 (folio, 225—296), was noted in the *Bibliographie de la France* in the issue of 27 N 1819, and so must have been issued some days or weeks before this. This seems to establish the priority of this fascicle over Volume 5 of the *Systema*, assuming that the latter was issued as a single volume at one time, rather than in separate fascicles in order of printing [21]). Priority in this particular case is important because of the numerous species of Rubiaceae and Campanulaceae (the latter mostly species of *Lobelia*) which were published in Fascicle 12 and were published slightly later, usually under different names, in the first 14 signatures of the *Systema*, volume 5.

Fascicle 13 of the *Nova Genera*, comprising pages 385—456 (folio, 297—356) was not published until March, 1820. About 8 species were published in this fascicle a few months after their publication in Volume 5 of the *Systema*:

Macrocnemum tinctorium R. & S. Syst. 5 : 6. ?D 1819; (*M. tinctorium* H.B.K. Nov. Gen. & Sp. 3 : 399. Mr 1820).
Dicrobotryum divaricatum R. & S. l.c. 221; (*Guettarda xylostioides* H.B.K. l.c. 328).
Anisanthus microphylla R. & S. l.c. 223; (*Symphoricarpus microphyllus* H.B.K. l.c. 331).
Gardenia humboldtiana R. & S. l.c. 243; (*Mussaenda pubescens* H.B.K. l.c. 320).

19) Flora 3 : 261. 7 May 1820.

20) Bull. Torr. Bot. Club 29 : 597. 1902.

21) I have not been able to learn which of these two courses was followed when the *Systema* was published. From notes which appeared from time to time in *Flora*, I suspect that the separate parts were distributed at least to certain persons or institutions as fast as they were printed. For example, the note of November, 1819, quoted above in the text, states that Volume 5 is nearly complete, and that the first fascicles of Volume 6 are already in the hands of the correspondents [*Einsenders*]. Possibly newly printed fascicles were sent out to authors of articles or authors of reviews, but were not generally distributed. In the absence of specific information on this point it would seem that, in accordance with Article 40 of the International Code, we should assume that the volume was published all at one time.

Lonicera gibbosa R. & S. l.c. 257; (*Xylosteum mexicanum* H.B.K. l.c. 332).
Sabicea villosa R. & S. l.c. 265; (?*S. hirsuta* H.B.K. l.c. 325).
Duhamelia odorata R. & S. l.c. 267; (*Hamelia suaveolens* H.B.K. l.c. 323).
Nauclea tomentosa R. & S. l.c. 221 appears to have been published prior to *N. aculeata* H.B.K. l.c. 298 (folio text), but subsequent to *N. aculeata* H.B.K. l.c. 382 (quarto text).

In addition to the above, there were published in this volume of the *Systema* about 40 species for which Kunth was unable to give equivalents.

Volume 6 of the *Systema* was published probably in the Autumn of 1820; the preface is dated at the Calends of August, and there is no reason to suppose that publication was much delayed beyond this. Some new species based on Humboldt and Bonpland specimens are described in this volume, but there is no confusion with the *Nova Genera* after the Spring of 1820. Volume 4 of the *Nova Genera* was published in 1820 in five fascicles (Ap, My, Jl, S, D) but this volume dealt with Compositae only, and this family was never treated in this edition of the *Systema*. The remaining volumes (5 to 7) of the *Nova Genera* were published in parts from 1821 to 1825, but the next volume of the *Systema* (part 1 of Volume 7) was not published until 1829.

In addition to the volumes of the *Systema* proper, three supplementary volumes (*Mantissae*) were issued by J. A. Schultes and his son J. H. Schultes. The first *Mantissa* (1824) was intended to bring up to date the first volume of the *Systema Vegetabilium*, and the second *Mantissa* (1824) was intended to perform a similar function for Volume 2 of the *Systema*. So far as I am aware no new Humboldtian species were published in either of the first two *Mantissae*, but in the third volume (1827) the authors proposed about 47 new species based on Humboldt specimens in the Willdenovian herbarium. Apparently this brought about no new conflict with the *Nova Genera et Species*, which had been completed in 1825, but Kunth, as pointed out above, published in 1830 a list in which he equated the species of the *Nova Genera* with those of Schultes and Schultes. He was able to identify, presumably from descriptions only, about three-quarters of the species. Nine of these published in the *Mantissa tertia* were completely unknown to him, and were perhaps not represented by specimens at Paris.

In this connection it should be emphasized again that in addition to the relatively few R. & S. names which have priority over H.B.K. names of which they are exact synonyms, there is a body of perhaps a hundred species proposed in the *Systema* or in the *Mantissa tertia*, which are unquestionably valid from the standpoint of nomenclature because they are based upon unique specimens in the Willdenow herbarium; these are the species which Kunth was unable to equate with any of his species in the *Nova Genera*, and which are presumably not represented by specimens in the Paris herbarium. Finally there are at least a few cases, and perhaps more than this, in which the Bonpland specimen seen by Kunth at Paris turns out to be not the same as its supposed equivalent in the Willdenow herbarium. Wimmer, for example, has pointed out [22]) that the type of *Lobelia ayavacensis* Willd. ex R. & S. was a mixture, only part of which represented *Lobelia umbellata* H.B.K. with which Kunth equated it.

SUMMARY

The American plants collected by Humboldt and Bonpland were deposited in the Muséum d'Histoire Naturelle, Paris, except for a large set given by Humboldt to Willdenow, who engaged to study the collection and write it up for publication. At Willdenow's death in 1812 this task devolved upon Kunth, who worked almost

22) Pflanzenreich IV. 276b (Heft 107) : 376. 1953.

entirely with the collection at Paris, upon which the descriptions in the *Nova Genera et Species* of H.B.K. were based. Willdenow's herbarium was controlled for a time by Schlechtendal, who permitted Roemer and Schultes, and others, to use and publish Willdenow's manuscript names and brief descriptions. This resulted often in multiplication of synonymous names because of nearly simultaneous publication, particularly by Kunth in the *Nova Genera* and by Roemer & Schultes in the *Systema Vegetabilium*. The most critical dates of publication, from the standpoint of priority in nomenclature, are those of Volume 3 of the *Nova Genera*, and Volumes 4 and 5 of the *Systema*. It is shown that Volume 4 of the *Systema* was probably published after Fascicle 10, and before Fascicle 11, of the *Nova Genera*. It is also shown that Volume 5 of the *Systema* was probably published after Fascicle 12, and before Fascicle 13, of the *Nova Genera*. Numerous species described in the *Systema* were not identified by Kunth with any of his own species, and it is concluded that these were based on unique specimens represented in the Willdenow herbarium only.

AFTERWORD BY AUTHOR

The foregoing account has been written in an attempt to call attention to the fact that most H.B.K. names appear to have priority over the R. & S. names with which they are synonymous. It is difficult in America, however, to do the bibliographic research necessary to fix more certainly the dates of publication of the *Systema Vegetabilium*, and it is to be hoped that some European taxonomists, by the use of contemporary literature, contemporary library accession records, or otherwise, can establish these dates as well as the manner of publication. It would also be interesting to know more about the circumstances which prevented Kunth from making full use of the Willdenow herbarium.

POUR UNE SIMPLIFICATION ET UNE NORMALISATION DES DIAGNOSES

par

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L'accroissement constant du nombre des espèces végétales décrites, l'imprécision de certaines descriptions, la difficulté de pouvoir consulter les très nombreuses publications de botanique parues ou paraissant actuellement dans le monde, l'isolement de certains chercheurs ou plutôt leur manque de liaison etc....., rendent chaque jour la tâche du Botaniste-descripteur plus ardue.

Quant aux déterminations de plantes, elles sont longues, parfois difficiles surtout si l'on n'a pas le "type" sous les yeux. Et l'on a connaissance de nombreux herbiers non encore déterminés. Le mal réside non en une pénurie de spécialistes mais surtout à mon avis, dans la manière actuelle dont sont établies la Taxinomie et la Systématique.

Toutes les familles sont nettement définies et les genres sont pour la plupart facilement discernables bien que les botanistes ne soient pas toujours d'accord et que toute combinaison ou classification préconisée par l'un soit invariablement combattue par quelques autres. La difficulté s'accentue lorsque l'on touche aux espèces. Les longues diagnoses sont loin de réunir tous les suffrages. Le différend porte souvent sur des espèces mal conçues. Il en est pour lesquelles on aurait peine à trouver un caractère distinctif valable. Quant aux autres caractères énumérés ils sont soit présents dans les espèces voisines soit d'une variabilité déconcertante. L'auteur ayant observé un seul échantillon était de bonne foi; la variabilité n'apparaît souvent en effet que sur un certain nombre d'échantillons réunis. Il existe enfin dans beaucoup de diagnoses des erreurs d'observation ou d'interprétation que l'on retrouve recopiées de "flore en flore".

Après avoir reconnu tous ces défauts parmi tant d'autres je me suis demandé si une simplification et une normalisation des diagnoses ne faciliteraient pas le travail de ceux dont la tâche est de reconnaître et de déterminer les plantes. Je conçois en effet que les descripteurs se laissent griser par le plaisir

de signer de nouvelles diagnoses, d'enfanter et de baptiser. Mais que pouvons-nous attendre de tels travaux irraisonnés et insuffisamment concertés?

La vieille question si souvent développée depuis Linnee et Mendel et à laquelle on a attribué le mal de l'incertitude en taxinomie; est celle des limites de l'espèce.

sur l'unite taxinomique

a) *Les limites des Taxons.*

Faire de grandes espèces, ou rassembler, ou faire de petites espèces ou pulvériser? Les nombreuses controverses qui eurent ce thème pour objet n'ont eu pour cause que l'incertitude du raisonnement.

La nature n'est pas formée de séquences au point que l'on puisse tailler dans chaque famille, les tribus, les genres et les espèces.

Les systématiciens aguerris arrivent à posséder la notion de "genre" à un tel point qu'ils trouvent du premier coup la faille entre tel groupe d'espèces et tel autre; certains ont ce que l'on peut appeler du "flair", mais d'autres tombent dans la facilité. La "pulvérisation" est le mal dont souffre trop souvent la systématique.

De nombreux systématiciens travaillent isolément et se fient aux diagnoses publiées par leurs confrères. Or, une diagnose ou un dessin d'ensemble doit être vu à travers l'esprit de son auteur. Certaines diagnoses sont concises et précises à la fois; d'autres sont au contraire longues et vagues, et l'on se demande après avoir parcouru une ou deux pages de texte, quel peut être le caractère distinctif qui permette une distinction avec l'espèce voisine. Les dessins d'ensemble sont exécutés le plus souvent par des dessinateurs professionnels qui interprètent plus ou moins fidèlement les indications qui leur ont été données par le botaniste.

Très souvent, des espèces sont créées par des botanistes qui méconnaissent un groupe, ou les travaux d'un collègue étranger. Ces espèces, dès publication de leur diagnose, sont mises en synonymie ou bien elles encombrent inutilement les flores pendant de longues anneés.

Les espèces, les variétés, les races sont définies par un ensemble de caractères morphologiques, anatomiques, chimiques ou cytologiques. Le nombre de ces caractères est parfois si important que l'on a peine à s'y reconnaitre, et l'on s'interroge encore sur les limites à donner aux genres et aux espèces dont certaines ont la valeur d'une variété voir même d'un jordanon (1-p. 4-25).

Pour Cuenot "*Une espèce indiscutable pour tout le monde est une réunion d'individus apparentés, ayant même morphologie héréditaire (morphologie externe et interne, nombre et structure chromosomique) et même caractères physiologiques: soit biochimiques (par ex. odeurs, sécrétions), soit biophysiques (réaction au milieu) ils ont un genre de vie commun et occupent une aire géographique définissable*".

"*En bref une bonne espèce se reconnaît à trois couples de critères: morphologie et physiologie, écologie et distribution, fécondité intérieure et stérilité extérieure; si nous appelons respectivement M, E et S les trois couples, une espèce indiscutable rèpond à la formule symbolique M E S. Les cas litigieux sont ceux où il manque un ou deux critères, par exemple lorsque des formes bien séparées par leur morphologie et leur écologie sont cependant interfécondés (formule ME)* (Cuenot, *1 p. 253). Nous n'hésitons pas à appeler espèce, toute forme qu'il est utile de séparer de ses voisines, parce qu'elle en diffère par deux critères au moins (ME ou ES)*" (Cuenot, 1, p. 254).

Vue par les taxinomistes l'espèce est essentiellement définie par les caractères Morphologiques et Ecologiques. Au contraire pour les génétistes c'est l'interfécondité qui constitue un argument décisif. Les caryologistes attribuent à la structure chromosomique un rôle arbitre et les biochimistes voient dans la constitution pondérale et qualitative de la matière les facteurs héréditaires sur lesquels peut se baser la classification des végétaux.

Et voici la définition générale de l'espèce donnée par Cuenot (1 p. 11) "*L'espèce est donc ce que l'on peut reconnaître à des traits descriptifs considérés comme permanents qui la séparent d'autres formes analogues*". Enfin, selon H. Baillon (Dictionnaire de Botanique) on distingue des genres "naturels" et des genres "moins naturels".

On ne s'étonnera pas si l'on en est encore après cela à chercher les limites des espèces et des genres. Un groupe de botanistes a synthétisé en un ouvrage (2) toutes les tentatives réalisées jusqu'à ce jour pour arriver à ce but.

A mon avis l'espèce est une vue de l'esprit. Cependant, puisque la nécessité oblige les hommes à tout étiqueter autour d'eux, force leur est de trouver des définitions, des descriptions pour justifier leurs appellations.

Parmi l'infinité des caractères que présente la nature vivante, certains se montrent avec plus de "relief" si je peux m'exprimer ainsi.

Ces caractères sont autant de "points de cristallisation", autour desquels on rassemblera après, des caractères de moindre importance, ayant moins de relief. L'ensemble constituera la description ou diagnose; mais si la "cristallisation" est poursuivie il vient s'assembler autour des premiers caractères des éléments de moins en moins purs ou de moins en moins caractéristiques. Au-delà d'une certaine mesure la quantité de ces dernières impuretés est telle, que les éléments purs y sont noyés et ne peuvent plus être discernés. Les difficultés de la classification ne peuvent donc être résolues que par une élimination réfléchie de ces caractères secondaires et une mise en évidence des caractères de "relief" précités.

b) *De l'importance du Taxon comme "modèle-étalon" — Les caractères génésiques.*

Dans une précédente publication (3- p. 85-86) j'avais émis l'opinion d'une restriction du nombre des caractères, surtout des caractères fluctuants afin d'éviter les confusions qui ne cessent de se produire.

Une classification basée sur un grand nombre de caractères secondaires, d'importances variables et souvent inutiles, ne peut être claire. "Une porte doit être ouverte ou fermée". Il est nécessaire de faire appel à plus d'intransigeance pour éviter les erreurs toujours possibles et les incertitudes dans lesquelles certains tendent à se complaire pour assurer une pérennité sur des nominations d'espèces établies sur aucun fondement solide.

Les taxinomistes oublient trop souvent qu'un "taxon" est une unité ou mieux un "modèle-étalon" auquel les collecteurs tenteront de rapprocher et de comparer pour les identifier, les échantillons de leur récolte.

La création de ces "modèles-étalons" entraîne une responsabilité qui n'est pas toujours ressentie par leur auteur. Pour s'assurer de l'importance du taxon il suffit de le comparer cependant à des modèles tels que le mètre et le kilogramme et de voir avec quelles précautions on a créé et homologué ces unités nouvelles. L'espèce, le genre sont au règne végétal ce que le mètre et le kilogramme sont au système métrique.

Un *modèle-étalon* pour être facilement utilisable doit être sérieusement et clairement établi. Sa valeur de modèle dépend de la stabilité des caractères ayant servis à le définir.

Ce sont ces caractères en "relief" que j'ai rapproché aussi des points de cristallisation d'une liqueur saturée, caractères les plus marqués, et avant tout les plus stables, qui serviront, de préférence à tous autres, à définir le nouveau modèle-étalon (genre ou espèce).

Ce sont d'ailleurs un ou plusieurs de ces caractères qui, dominant les autres, ont attiré l'attention du descripteur. Ce sont ce ou ces caractères distinctifs épaulés par quelques caractères découverts après coup par le descripteur (mais tout aussi valables) et qui l'ont conduit à instituer le nouveau "taxon", que j'appelle "caractères génésiques ou de génèse".

Le caractère génésique est un caractère distinctif de la famille, du genre ou de l'espèce, que l'on ne devra pas confondre avec les caractères génériques ou spécifiques attribués aux genres et aux espèces dans les diagnoses ou les descriptions des actuelles Flores et peuvent être des caractères secondaires variables approximatifs ou complémentaires dans lesquels on trouvera par exemple les expressions: environ, fréquemment, etc.... ou des caractères divers biologiques, de coloration, etc. qui ont pu être observés par le collecteur sur la plante vivante mais qui ne sont d'aucun secours pour la détermination d'un échantillon sec d'herbier.

Les taxinomistes doivent donc considérer deux groupes de caractères:

1/ — *Les caractères génésiques invariables* définissant le "type ou étalon taxinomique" et devant constituer la véritable diagnose,

2/ — *Les caractères variables* (nombre de caractères spécifiques actuels par exemple) définissant les limites de variation de l'espèce qui constituent le plus souvent la plus grande partie de la description connue habituellement sous le nom de diagnose mais dont l'appellation véritable devrait être "description morphologique".

DE L'UTILISATION DES CARACTERES GENESIQUES ET DE L'UNITE TAXINOMIQUE

a) *De la description à la véritable diagnose taxinomique.*

Les longues diagnoses sont de lecture fastidieuse parce qu'elles ne sont pas seulement des diagnoses au sens véritable mais des descriptions morphologiques dans lesquelles on trouve énumérés les caractères du genre de la tribu voir même quelquefois de la famille et pêle-mêle toutes les observations de l'auteur. Ecrire dans la diagnose de l'espèce "*Juglans regia* L." que cette plante est un arbre est parfaitement inutile parce que tous les *Juglans* sont des arbres et que ce caractère n'est pas particulier à l'espèce *J. regia* L.

Le caractère "arbre" peut donc tout au plus servir comme caractère génésique de famille puisque toutes les Juglandacées sont des arbres.

A mon avis on devrait éviter aussi d'insérer dans une diagnose des caractères génésiques observables sur le terrain à moins qu'ils ne soient apparents aussi sur l'échantillon d'herbier.

L'espèce "rubra" si elle n'est basée que sur le seul caractère rouge des tiges du pétiole, voir des fleurs, sera bien difficile à reconnaître sur un échantillon sec et brun d'herbier, s'il n'y a pas d'autres caractères génésiques indestructibles par les méthodes actuelles de préparation des échantillons.

La diagnose constituée des caractères distinctifs choisis notera également le binôme de l'espèce affine.

La diagnose ainsi conçue n'enlève rien à la description morphologique qui pourra suivre et qui pourra servir comme moyen de confirmation.

Un genre ne doit donc être défini que par un ou deux ou tout au moins un nombre restreint de caractères de génèse, étant entendu que ces caractères sont reconnus comme stables. On sait déjà sous ce rapport que les caractères floraux sont plus stables que les caractéres des organes végétatifs.

On admettra donc pour chaque genre un ou plusieurs caractères clefs appelés "caractères génésiques ou de génèse", seuls valables, les caractères variables seront éliminés. Les caractères secondaires n'auront qu'une valeur confirmative dans la détermination.

Les espèces seront elles-mêmes basées sur un ou au plus trois caractères de génèse.

Seules les diagnoses variétales ou raciales pourront faire appel à des caractères fluctuants (Dimensions, formes mal définies, coloration, caractères variables, etc. . . .).

Il sera ainsi possible de rassembler la totalité des genres et leurs variétés en un ouvrage ayant force de loi, ou générique dont il sera parlé ultérieurement.

La création d'un genre ne pourra être acceptée que sur désignation d'un ou au plus trois caractères génésiques invariables, c'est-à-dire stables permettant de distinguer à coup sûr le genre ou l'espèce allèle ou affine avec lequel il était précédemment confondu et d'avec lequel ou laquelle l'auteur a cru bon de l'en distinguer.

Les caractères variables dimensionnels ne seront pas acceptés comme caractères génésiques à moins qu'ils ne soient nettement tranchés. On tendra par conséquent à éliminer les caractères définis par les expressions: environ, plus ou moins, assez souvent, fréquemment, etc. . . . qui ne sont pas absolues.

Lorsque les caractères génésiques sont choisis pour chaque espèce ou genre, il conviendra ensuite aux spécialistes d'en vérifier la valeur au cours de leurs travaux.

Supposons donc l'espèce A et un groupe d'échantillons que l'on hésite à classer dans cette espèce à cause de quelques caractères aberrants. Cette disparité incitera le botaniste à décrire d'après ces échantillons l'espèce B — et notera les caractères génésiques qui l'ont déterminé à cette distinction. Or, d'autres récoltes montrent par la suite que certains échantillons présentent à la fois des caractères de A et B sur le même rameau ou sur la même plante. Il conviendra donc de dénoncer les caractères génésiques de B et de procéder officiellement à la suppression de cette nouvelle espèce.

Les caractères de famille ne seront pas adoptés comme caractères génésiques de tribu; de même les caractères génésiques de tribu ne pourront être adoptés comme caractères génésiques d'espèce.

b) *La détermination des espèces.*

La concision des diagnoses des familles, des genres et des espèces conduiront ainsi plus rapidement le botaniste vers le résultat désiré à savoir, la détermination d'un échantillon.

Si l'échantillon est incomplet, les organes sur lesquels portent les caractères génésiques peuvent manquer. Dans le doute le botaniste ne peut alors se baser que sur la description morphologique pour accumuler le maximum d'arguments en faveur du classement de son échantillon. La détermination se borne alors à un rapprochement intuitif. Le binôme adopté sera suivi d'un point d'exclamation indiquant que la véritable identification n'a pu être faite. Ex.: Triticum vulgare ! L.

CONCLUSIONS

Il serait donc souhaitable:

1°) De distinguer dans les descriptions d'espèces ce qui est du domaine de la Taxinomie et la Systématique pures et ce qui est du domaine de la Morphologie générale. Il y aurait lieu d'observer un certain rigorisme et une certaine concision dans les diagnoses pour ne pas avoir à regretter par la suite les erreurs et les pertes de temps auxquelles peuvent conduire l'adoption de caractères instables et l'imprécision.

2°) A cet effet on adoptera et l'on mettra

en évidence les seuls caractères systématiques ayant conduit à la distinction du nouveau Taxon de son espèce affine et l'on notera cette espèce affine dans la diagnose. Ces caractères en "relief", stables, seront appelés "caractères génésiques ou de génèse".

3°) L'application des statistiques mathématiques dans le but d'élever le caractère morphologique variable au rang de caractère génésique ne peut avoir pour résultat que d'en fixer la limite de variation et le degré d'incertitude.

4°) Le nouveau taxon décrit sur la base d'un ou de plusieurs caractères génésiques doit être considéré comme un "modèle-étalon".

5°) Pour tout échantillon non conforme par manque des organes sur lesquels peuvent être observés les caractères génésiques, le déterminateur, sur son "determinavit", doit faire suivre le binôme adopté, d'un point d'exclamation indiquant que le rapprochement effectué n'est qu'intuitif. Ex.: Triticum vulgare ! L.

Une description ne doit pas être rédigée pour le seul usage de son Auteur. Elle doit à mon avis être facilement utilisable et accessible à n'importe quel Taxinomiste voir à n'importe quel botaniste collecteur ou herborisant.

Je pense enfin qu'au XXème siècle où nous sommes, le siècle du progrès scientifique et de la vitesse, la botanique doit se mettre au diapason des autres disciplines scientifiques et que l'on doit rechercher une amélioration constante de ses méthodes d'investigation afin que les déterminations, tout en conservant la certitude qu'elles exigent, soient aussi plus faciles et plus rapides. C'est la raison pour laquelle je pense qu'une simplification et une normalisation des diagnoses seraient indispensables et cela serait possible par l'emploi et la mise en évidence des caractères primordiaux ou génésiques.

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Zwei unbekannte Linné-Briefe botanischen Inhaltes

von

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Obwohl die zwei zu veröffentlichenden Briefe von Linné bereits ein Mal publiziert wurden, wage ich es dennoch zu behaupten, dass sie bis heute der Linnéforschung unbekannt blieben. Weder Ährling (1885) noch Soulsby (1932) erwähnen die einschlägige Stelle. Was aber ganz merkwürdig ist: selbst Th. M. Fries, der doch alle Fäden, die den bekannten Briefwechsel von und an Linné zusammenbanden, in seiner emsigen Hand hielt, hatte bei Behandlung der beiden Bände mit dem Anfangsbuchstaben „B" keine Kenntnis von der Existenz jener beiden Briefe. [1])

Und wie kann man auch auf den Gedanken kommen, ausgerechnet in einer rein theologischen Schrift über die Lebensgeschichte unseres Erlösers versteckt, in Fussnoten, die beiden Briefe zu finden?

Der Verfasser jenes Wälzers heisst Sven Bælter. Zunächst über Bælters Persönlichkeit und seine Beziehungen zu Linnæus. Bælter wurde am 24. Juli (alt. St.) 1713 in Söderhamn geboren, wo sein Heim später durch eine Feuersbrunst in Asche gelegt wurde, sodass das Elternhaus in Not geriet. Als frommer Theologe wurde er später Magister und mit 32 Jahren in den Priesterstand eingeweiht. Er avansierte so allmählich zum Hofprediger, dann zum Gesandtschaftsprediger in St. Petersburg und schliesslich zum Domherrn in Växjö (1750), wo er den 19. November 1760 starb. Bælter hat eine sehr reiche literäre Produktion geistlicher Schriften hinter sich, deren Bibliographie in der zweiten Auflage seiner Messiade (1770) vier Grossquartseiten umfasst, die in der dritten Auflage weggelassen ist. Sein Stan-

1) Selbst in der akademischen Abhandlung über Sven Bælter von Johan Dellner (Uppsala 1916) werden die beiden Linné-Briefe ignoriert.

dardwerk ist „die historischen Anmerkungen über die Kirchenzeremonien" in schwedischer Sprache. Man weiss nicht, wann Linnæus mit ihm bekannt wurde; wahrscheinlich dürfte er Bælters Predigt in der Domkirche zu Uppsala 27. I. 1743 beigewohnt haben; sicher aber war Linnæus zugegen, als er am 29. Januar 1744 in Anwesenheit der königlichen Hoheiten abermals dort predigte. (Diese Predigt wurde im Auftrage der Königin-Mutter Albertina Friederike auch ins Deutsche übersetzt.)

In der Korrespondenz mit seinem besten Freunde, dem Archiater Abraham Bäck, findet man einen Monat später einen Brief von Linnæus folgenden Inhaltes:

„Seine Kgl. Hoheit war bei uns im Januar, war in der Orangerie und besuchte mich in meiner Kammer, besichtigte meine kleine Sammlung von Naturalien; da wurde auch Magister Bælter zu seinem [ausserordentlichen] Hofprediger ernannt." (2. III. 1744, Brief n. 63, p. 13).

Am 12. Mai desselben Jahres schreibt er wieder an Bäck:

„Alle Decouverte, die wir erfuhren, sind traurig. Dass Magister Bælter mit dem Ambassadör nach Russland fahren soll, alsbald nachdem er zum Hofprediger von Seiner Kgl. Hoheit ernannt wird, dass Prof. Anders Celsius mit dieser Welt sein Observatorium quittiert hat und dass Sie, Hr. Dokt., mutterlos wurden." (Brief n. 634k p. 24).

Dass Linnæus die beiden Trauerbotschaften, den Tod seines Jugendfreundes, des berühmten Astronomen Anders Celsius und der Mutter seines intimsten Freundes Abraham Bäck mit der Versetzung von Bælter nach St. Petersburg als einen tiefen Schlag empfand, spricht deutlich dafür, wie hoch er den neugebackenen Botschaftsprediger schätzte.

Auch der am 25. April 1744 erfolgte Tod von A. Celsius, dem übrigens Linnæus die Erfindung der h u n d e r t g r a d i g e n Thermometerskala strittig machte, steht in einem gewissen Zusammenhange mit Sven Bælter, ein Zusammenhang, der Linnæus nicht unbekannt gewesen sein dürfte. Als der Geistliche am Sterbelager von Anders Celsius tröstend über die Unsterblichkeit der Seele und das Leben im Jenseits sprach, soll ihm der sterbende Freidenker geantwortet haben: „Sagen Sie das, Herr Magister. No, bald komme ich in den Zustand, dass ich zu sehen bekommen werde, was davon wahr ist und was nicht". (Hildebrand, p. 497).

Eigentlich konnte Linnæus über Bælters Versetzung nach St. Petersburg in einer Hinsicht froh sein. Linné, der in jedem im Auslande verweilenden Bekannten zunächst ein Subjekt sah, das seine Naturaliensammlung womöglich um Pflanzen, Samen und Tiere bereichern könnte, hatte auch Grund derartige Sendungen von ihm aus Russland zu erwarten. Und er wurde in seinen Erwartungen nicht getäuscht.

In seiner Beschreibung der Reise nach Russland, die er als MS dem bekannten Bibliothekar Gjörwell (1759) überlassen hat, berichtet Bælter über seinen Besuch bei J. G. Gmelin in Petersburg, den berühmten Erforscher Sibiriens (vgl. Otto Gmelin: Johann Georg Gmelin 1709-1755):

„Er gab mir manche sibirische Sämereien, die ich dem Archiater Linnæus schickte, wie auch drei lebende, seltene Pflanzen in Töpfen, die ich für den Akademie-Garten in Upsala mitbrachte". (Swederus 1877, p. 67 Fussnote 1).

Da Bælter bereits am 13. September 1743 in seiner Heimat eingetroffen war, so müsste er hiernach bereits spätestens Mitte 1743 seine Korrespondenz mit Linnæus eingeleitet haben. Gmelin war da seit 28. Februar 1743 wohlbehalten nach einer Abwesenheit von 9½ Jahren aus Sibirien zurückgekehrt.

Nach dieser informierenden Einleitung lasse ich den Wortlaut der beiden ganz in Vergessenheit geratenen Briefe von Linnæus folgen. Eigentlich war es für Bælter überflüssig, sich in den naturgeschichtlichen Fragen, die irgendwie mit dem Leben Jesu im Zusammenhange standen, an Linnæus zu wenden, da die Schriften von Borchart (Hierozoicon), O. Celsius d. ä. (Hierobotanicon) und die von Linnæus vor Kurzem, posthum herausgegebene, Reise nach Palästina von HasselQuist (Iter palæstinum, 1757), die von Bælter wiederholt angeführt werden, ausreichend Auskunft gaben. Es ist möglich, dass Bäck den Verfasser vom Leben Jesu ermutigt hat, sich in noch nicht ganz geklärten Sachen, an Linnæus zu wenden; dafür könnten zwei ebenfalls unbekannte Briefe von Bäck, der erste vom 2. Juli 1757 (Bælter, p. 997) über Christi Blutschwitzen sprechen. Der zweite über den Speerstich während Christi Passion auf zwei Quartseiten trägt allerdings das Datum vom 20. Mai 1758 (l.c. p. 1257-1258), damals war bereits die Korrespondenz mit Linnæus eingeleitet.

„Herr Archiater und Ritter *Carl Linnæus*", so beginnt in der Fussnote von Seite 738 Bælter, „schreibt darüber an mich den 20. Januar 1758 sohin."

„Herr *Doctor* behagen in seiner hochgeehrten Anfrage, wie *Sycomorus* etwa auf Schwedisch heisse, worauf ich ergebenst Antwort geben sollte, aber ich gestehe redlich, dass ich dies nicht weiss, zumal dieser Baum so selten ist, dass er kaum in einem oder zwei akademischen Gärten in Europa gesehen wurde. Man muss daher wohl mit diesem so machen wie mit manchen anderen, die von ausserhalb zu uns gelangten, und die lateinischen und griechischen Namen behalten und ihn *Sycomor* benennen, ebenso wie *Syren* „(= Flieder)", *Cypresse, Kastanie, Persica,* „(= Pfirsich)" *Dattel, Jasmin* etc. vorher hiessen; denn obwohl Sycomorus eine *Species* von Feigen ist, mit untauglicher Frucht zu essen, würde es doch mehr *obscur* werden, falls man ihn Blattfeige, Ägyptische Feige oder anders benennen würde". (Wegen *Ficus Sycomorus* vgl. Spec. pl. (ed. I), II, p. 1059).

Der Inhalt des zweiten veröffentlichten Briefes an Linnæus ist bei Weitem viel merkwürdiger. Er bildet die Fussnote zu folgendem Originaltexte. Bælters **Text** lautet: „Aloe ist ein indianischer Baum, der mit Gold berechnet wurde und an Könige und Fürsten als teuere Geschenke (Soll von Aloe (*Aloë Catharica*) unterschieden sein, hat einen bitteren Saft; man findet sie in den Apotheken) geschickt wurde. Aus Indien wurde sie nach Ägypten und Palästina eingeführt, wo sie wenigstens zu Salomos Zeiten gepflanzt wurde. Sie gibt einen starken und schönen Duft und wird mitunter unter gewissen Spezereien angeführt, mit welchen die Orientalen ihren Körper, ihre Kleider und Häuser zu räuchern pflegen, weshalb man sie auch *Weihrauchbaum* nennt. Diese wohlriechende Aloe des Nikodemus war mit der kostspieligen Myrrha gemischt, die zusammen eine Salbung bildete, die teuer und ausgibig war." (Ed. I, Stockh. V. Buch p. 672. 1759).

„Hierüber hat Herr Archiater und Ritter Linnæus durch einen Brief vom 23 Mai 1758 geruht, mir seine Gedanken so mitzuteilen."

„Die *Myrrha* der Alten ist gerade dieselbe *myrrha* und jener Harz, der heutzutage in den *Apotheken* gebraucht wird, sehr scharf und widersteht aufs Höchste der Fäulnis, wie alle *amara,* weshalb sie zu allen Zeiten, und noch heute, zur Balsamierung verwendet wird; und die alten *Mumien* der Ägypter sind mit ihr, ausser mit was anderem zubereitet, balsamiert. Von wo die Myrrha kommt, oder welcher Baum dazu genommen wird, das haben die Alten zu *notieren* versäumt und die Neueren haben es vergebens versucht herauszubekommen. Ich kam darauf auf die Spur durch ein prächtiges Stück, das der sel. Doct. Hasselquist in Ägypten erhielt, und das sich nun im *Cabinette* Ihrer M-st. der Königin befindet. Dort sieht man deutlich, dass es nicht als *gummi* oder *resina* von einem gewissen Baume sondern eine Ansammlung von *Insecten* ist, auf gleiche Weise wie *gummi Laccae.* Sie sitzen um einen trockenen Ast herum, wo es nicht *extraversiert* werden konnte, sondern nur wenn man ihn zerbricht, findet man voll von *transversalen Poren,* und in jeder einzelner *Pore* ein kleines *Insect.* Man sieht es deutlich, dass Insecten diese *resina* gesammelt haben, sie um den Ast herum gesetzt haben, dass sie darin ihre Jungen legen, gerade auf gleiche Weise, wie in *gummi laccae. Tinctur* aus *myrrha* ist sonst eines der feinsten *cosmeticis,* um die Haut weiss zu machen.

Aloë ist dasselbe, was man in Apotheken *lignum Aloes* nennt, obwohl man es nicht immer *veritabel* erhält. Der Baum ist, falls man *Rumphius* glauben soll, der selbst in Indien war, *Ficus parvifolia.* Man verkauft es in China zum doppelten Preise des Silbers, dem Gewichte nach, und verwendet es als Weihrauch in *templis deorum* und *palatiis regum.* Das ist nur der Kern der allerältesten Bäume. Im Übrigen ist die Beschreibung des Hn. *Doctors* ziemlich *accurat.*" (Ed. I, VI. Buch p. 632-633, Stockholm 1759).

Zunächst zur Myrrha. Linné hat in einer kleinen, posthum erschienenen Studie (vgl. Sv. Linné-Sällsk. Årsskr. 1945, XXVIII, p. 65 ff) über die Balsamierungskunst Myrrha als Ingredienz bei der Behandlung der Leichen überhaupt nicht erwähnt. Blumenbach gibt als Konservierungsmittel im Kapitel VIII „Ingredienzen zur Mumienbereitung" Natrum, Cedria (als Baumharz von der Libanon-Zeder, *Pinus cedra*) und Asphalt an. Sein Wissen um Myrrha hat Linné aus S. Dales Pharmacologia geschöpft; dessen originelles Schmetterlingssystem er sich bereits zuvor — gelinde gesagt — angeeignet hat. Dale gibt an: „Myrrha offic. C. B. J. B. Parck. Myrrh. Vires „putredini resistit." „Myrrham Officinarum pro Bdellio habuerunt & revera tanta intercedit inter Bdellium & Myrrham similitudo, ut facilè ab incautis et minus expertis unum pro alero accipi possit". Linné selbst hat in keiner seiner Auflagen von Species plantarum, Genera plantarum oder Systema Naturae die Myrrha behandelt, schon aus diesem Grunde, weil er nicht wusste, wie die Pflanze aus-

sieht, sohin keine Diagnose geben konnte. Aber in seiner „Materia medica" wird das Harz der Myrrha (l.c., p. 235 n. 558) erwähnt, bei Angabe der „Vis": u. a. „balsamica, ... anthelminthica, antipudridinosa". „Loc.: Regio Trogolodytarum Africae".

Noch bei Linnés Lebzeiten hat J. F. Gmelin (1774, p. 344-345) sich ausführlich über die Myrrha ausgelassen: „Myrrha, Myrrhen, so nennt man einen harzigen, verdickten Schleimsaft, welcher aus Africa und Asien zu uns gebracht wird und dessen Pflanze uns bisher noch unbekannt ist."

Wie heisst nun heute die Pflanze?

Nach G. B. Winer komme dieser — fast legendäre — Baum in Palästina nicht vor, sondern in Ätiopien. „Erst Ehrenberg (1829) gab eine genaue Beschreibung des Gewächses, das nun *Balsamodendron Myrrha* Cl. heisst. Vgl. Nees v. Esenbeck, plant. officin. Taf. 357."

Demgegenüber steht die Angabe bei O. von Hovorka u. A. Kronfeld (p. 318), dass Myrrha das Gummiharz von *Balsamea Myrrha* Engl., einer Burserazee sei.

Nun zum *Ficus* (olim *Aloë*) *parviflora* L. In allen seinen „Katalogen", wie man Linnés grosse Nachschlagewerke verallgemeinert nennen darf, gibt es keine *Aloë* oder *Ficus* mit diesem Trivialnamen. Für einen Kenner von Linnés Gewohnheiten ist das imgrunde nichts Überraschendes. Er pflegte aus Zerstreuung oder slechthin, getrieben von seiner Benennungssucht, oft für ein und dieselbe Art den Namen zu variieren. Vielleicht hatte er *Aloë perfoliata* im Sinne? In seiner „Materia medica" (p. 96) gibt Linné für diese „Vis" u.a. an: „balsamica, anthelminthica", ähnlich wie für Myrrha. In den Apotheken heisse sie: *Aloë caballina.*

Der ganze Stil und Inhalt beider Briefe gibt einen Vorgeschmack von Linnés späterer Befassung als wissenschaftlicher Ratgeber in der Bibelkommission vom Jahre 1771, einer wissenschaftlicher Tätigkeit, die die „Gedanken zu den spitzfindigen und unfruchtbaren, aber mit tiefstem Ernste vorgetragenen Disputationen" (Bryk, 1944) hinüberleitet. U.a. den Taubendreck, den die Bewohner von Samaria während der Belagerung nach dem Buche der Könige gegessen haben sollten, deutete damals Linné als *Ornithogalum umbellatum* und seine Erklärung, dass Eva aus dem Steissbeine und nicht aus der Rippe Adams geschaffen wurde, sonst hätte Adam ad finitum (sic) selbst Kinder zeugen können, erinnert schon stark an infantile Anthropogonien, wie sie Freud und seine Schule so plastisch geschildert haben.

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Additional Notes Concerning the New Species Collected by Hélène, Duchess of Aosta, in Tropical Africa

by

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In 1952 we published a note [1]) in which we discussed a series of printed drawings in the herbarium of the Jardin Botanique de l'État at Bruxelles. These drawings, the publication of which was not recorded either in Index Londinensis or Index Kewensis, illustrate new species collected by Hélène, Duchess of Aosta, and described in 1913 by Buscalioni and Muschler [2]). At the time of publication of our note we had not had the opportunity to see the work of Piscicelli [3]) in which the drawings were published, and had received all our information through the kindness of Professor Dr. R. Pichi-Sermolli of the Istituto Botanico in Florence. Since that time one of us has had an opportunity to examine a copy of Piscicelli's book, which is in the Library of Congress, Washington, D. C., U.S.A. We think it will be useful now to publish a short discussion of the work and to list the species illustrated in it.

Piscicelli's book is divided into three parts entitled as follows: I, "Le Sorgenti del Congo"; II, "Le Sorgenti del Nilo"; and III, "Alle falde dell' Altipiano Abissinico". Each section is again divided according to geographic regions. In the first, the three regions discussed are: (1) Lake Banguelo, (2) Lake Kivu, and (3) Lake Tanganyica. Only under the first and third of these are descriptions and illustrations of new species of plants found. In the second part four regions are discussed and only under the first are new species of plants described and illustrated. The regions are: (1) Ruanda, (2) Lake Albert-Edouard, (3) Lake Albert, and (4) Lake Victoria Nianza. In the third part in which the "Altipiano Abissinico" is discussed there are also descriptions of new species with an illustration for each.

As we mentioned in our earlier note, in the publication of Piscicelli the species are described, and briefly discussed, in Italian; the drawings (one for each species) are not numbered, and the initials "G. B." found on the proofs at Bruxelles, do not appear on the published plates. Moreover, there is no reference either in the preface or in the text concerning the artist. The date of publication by Piscicelli was 25 November 1913, according to an announcement at the end of the book. This date is after the work of Buscalioni and Muschler, which appeared in March (Heft iii and iv) and June (Heft v) 1913.

A map showing the route of the expedition and two pages of explanation are bound into the volume after the page giving date of publication.

In the list given below some errors in the spelling of the names will be noted; we give them exactly as the author had them. The headings in the list are those given by the author (in Italian) for each region.

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1) Schubert, B. G. and Troupin, G. in Bull. Soc. Roy. de Bot. de Belg. 85 : 5-8. 1952 — A Propos des Espèces Nouvelles récoltées par la Duchesse Hélène D'Aoste en Afrique Tropicale.

2) Buscalioni, L. and Muschler, R., Engl. Bot. Jahrb. 49, Heft 3, 4, and 5 (March and June, 1913).

3) Piscicelli, Maurizio, Nella Regione dei Laghi Equatoriali, pp. ix + 479 (Nov. 1913). Naples.

Parte Terza

Alle falde dell' Altipiano Abissinico pag. 397
Di alcune specie botaniche nuove, trovate nell' Altipiano Abissinico dalla spedizione di S. A. R. la Duchessa d'Aosta pag. 457

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DI ALCUNE SPECIE BOTANICHE NUOVE TROVATE SUL LAGO BANGUEOLO DALLA SPEDIZIONE DI S. A. R. LA DUCHESSA D'AOSTA

[Footnote:]

N.B. Queste specie furono studiate e descritte in esteso dai Professori L. Buscalioni e R. Muschler. — Le note qui citate furono tolte dalla Rivista: *Botanische Jahrbücher für Systematik, Pflanzengeschichte und Pflanzengeographie*, di A. Engler (Sonderabdruck aus Band 49, Heft 3 u. 4).

Pages 74 & 75 Melothria pulchra [4])
„ 76 & 77 Erythrocephalum Helenae
„ 78 & 79 Sphachophyllum Helenae
„ 80 & 81 Eupatorium Helenae
„ 82 & 83 Aostea, gen. nov. and Aostea Helenae
„ 84 & 85 Aostea pulchra
„ 86 & 87 Vernonia Aosteana
„ 88 & 89 Thunbergia Castellaneana
„ 90 & 91 Barleria Casatiana
„ 92 & 93 Cissus Helenae
„ 94 & 95 Acalypha Helenae
„ 96 & 97 Polygala Engleriana
„ 98 & 99 Adenodolichos Helenae
„ 100 & 101 Argyrolobium Helenae
„ 102 & 103 Cleome pulcherrima
„ 104 & 105 Ritchiea Engleriana
„ 106 & 107 Pouzolsia Piscicelliana
„ 108 & 109 Dorstenia Piscicelliana
„ 110 & 111 Disa (Callostachys) Helenae
„ 112 & 113 Heleocharis Helenae
„ 114 & 115 Eulophia Piscicelliana
„ 116 & 117 Lepidagathis Lindaviana
„ 118 & 119 Psiadia pseudonigrescens
„ 120 & 121 Senecio Piscicellianus
„ 122 & 123 Erythrocephalum Castellaneanum
„ 124 & 125 Coleus Helenae
„ 126 & 127 Harveya Helenae
„ 128 & 129 Sabaudia, gen. nov. and Sabaudia Helenae
„ 130 & 131 Orthosiphon Helenae
„ 132 & 133 Orthosiphon de Gasparisianum

4) In all cases the description of the species is given on the verso (page paire), the illustration on the opposite (unnumbered) recto (page impaire).

Pages 134 & 135 Ocimum superbum
Page 237 [unnumbered]

DI ALCUNE SPECIE BOTANICHE NUOVE TROVATE SUL LAGO TANGANICA DALLA SPEDIZIONE DI S. A. R. LA DUCHESSA D'AOSTA

[Footnote:]

N.B. Queste specie furono studiate e descritte in esteso dai Professori L. Buscalioni e R. Muschler. — Le note qui citate furono tolte dalla Rivista: [reference as for previous footnote].

Pages 238 & 239 Antholyza De Gasparisiana
„ 240 & 241 Jaumea Helenae
„ 242 & 243 Physotrichia Helenae
„ 244 & 245 Dissotis Simonis Jamesi
„ 246 & 247 Dissotis De Gasparisiana
„ 248 & 249 Dissotis Helenae
„ 250 & 251 Eriosema pseudocajanoides
„ 252 & 253 Desmodium Helenae
„ 254 & 255 Tristachya Helenae and T. Pilgeriana
„ 256 & 257 Senecio Helenae
„ 258 & 259 Pycnostachys pseudospeciosa

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DI ALCUNE SPECIE BOTANICHE NUOVE TROVATE NEL PAESE ROANDA DALLA SPEDIZIONE DI S. A. R. LA DUCHESSA D'AOSTA

[Footnote: exactly as for other sections]

Pages 300 & 301 Biophytum Helenae
„ 302 & 303 Dissotis Emanueli
„ 304 & 305 Vigna Harmsiana
„ 306 & 307 Dicoma bangueolensis

Page 457 [unnumbered]

DI ALCUNE SPECIE BOTANICHE NUOVE TROVATE NELLA REGIONE FRA IL CHENIA E L'ALTIPIANO ABISSINICO DALLA SPEDIZIONE DI S. A. R. LA DUCHESSA D'AOSTA

[Footnote: exactly as for other sections]

Pages 458 & 459 Ochna Piscicelliana
„ 460 & 461 Crotalaria Helenae
„ 462 & 463 Cleoma Margaritae
„ 464 & 465 Barbacenia Helenae
„ 466 & 467 Crotalaria africana
„ 468 & 469 Pulicaria marsabitensis
„ 470 & 471 Plectranthus Margeritae
„ 472 & 473 Plectranthus Emanueli
„ 474 & 475 Herderia niiroensis

Page 476 [unnumbered] blank
Pages 477, 478, 479 — Index
Page 480 [unnumbered] blank; opposite recto

— Table of Contents; following recto — Date of Publication — *"Finito di stampare il giorno 25 Novembre 1913 nello Stab. d'Arti Grafiche Luigi Pierro e Figlio in Napoli, Via Roma 402."* Following recto and verso — Explanation of Map.

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